Reflections on Stimulus Control

Murray Sidman
Sarasota, Florida

The topic of stimulus control is too broad and complex to be traceable here. It would probably take a two-semester course to cover just the highlights of that field's evolution. The more restricted topic of equivalence relations has itself become so broad that even an introductory summary requires more time than we have available. An examination of relations between equivalence and the more general topic of stimulus control, however, may reveal characteristics of both the larger and the more limited field that have not been generally discussed. Consideration of these features may in turn foster future developments within each area. I speak, of course, about aspects of stimulus control that my own experiences have made salient to me; others would surely emphasize different characteristics of the field. It is my hope that cooperative interactions among researchers and theorists who approach stimulus control from different directions will become more common than is currently typical.

Key words: stimulus control, stimulus discrimination, conditional discrimination, stimulus control topography

When Tim Hackenberg asked me to trace the evolution of key ideas and concepts in the realm of stimulus control and equivalence, I blanched. The last time I tried to do that, it required a semester-long seminar with oral reports on more than 30 publications, and supporting data and theory from many more. The last of those seminars took place a long time ago; since then, many new developments have occurred. I have to admit that I am no longer able either to acquaint myself with all the new developments or even to remember all the old ones. A long time ago it became impossible for me to know it all, so I had to become specialized. I console myself over that limitation with a truth that experience has taught me: The more deeply one goes into a specialized topic, the more one realizes how intimately that topic is related to everything else. Real specialization, although frowned on by so-called generalists, eventually brings one into contact with the rest of the world. One cannot really know how general anything is until one has gone into it deeply.

Address correspondence to the author at 3435 Fox Run Road, Unit 347, Sarasota, Florida 34231 (e-mail: murraysidman@comcast. net).

Much of the original work and thinking about stimulus control have become old hat, but there may still be some virtue in describing a bit of the context in which some of that early work was done and how it led to later developments. Then, too, the lack of follow-up of some of those early studies has been surprising to me. So let me just go over a few of my own experiences that contributed not only to my understanding of stimulus control but also to my appreciation of the role stimulus control plays— and could play—in other aspects of our science and even in our own lives.

What is stimulus control? How to define it? A general principle of our science is that what we do is controlled by reinforcing consequences; thus, we have operant behavior. A second general principle is that the environment comes to control operant behavior; that is to say, stimuli control relations between what we do and its reinforcing consequences. Skinner's three-term contingency encompasses both principles: Doing this, and not something else, produces that particular reinforcing consequence, but does so only in the presence of a particular environmental state. The three-term contingency brings the environment into the definition of behavior. That is be-

cause each of the contingency's three elements—stimulus, response, and reinforcer—can be defined only in terms of the others: An event can be called a stimulus only if changes in it lead to changes in a response; an event can be called a response only if it produces a measurable consequence and if it changes when some aspect of the environment changes; a consequence can be called reinforcing only if it produces a subsequent change in a preceding response. Taken together, then, the three elements define a unit of behavior.

In turn, three-term contingencies are themselves controlled by higher order relations among environmental elements. That is to say, the control exerted by a given stimulus in a three-term contingency comes itself under the control of other environmental elements. We call these relations conditional. How to characterize all these types of relations—the basic three-term contingency through *n*-term contingencies—how to measure and analyze them, and how to account for their development and maintenance constitute the domain of the experimental analysis of behavior. Our understanding of the response–reinforcement relation, however, is considerably more advanced than our understanding of stimulus control, particularly when we go from the three-term to the four-term contingency and beyond, that is to say, to conditional discriminations.

For example, in a simple light–dark discrimination, we may present food if a pigeon pecks a key that is lighted but not if it pecks a dark key. Suppose we then add a step to our procedure and make the light–dark discrimination conditional on the presence or absence of a new environmental element, a tone. When the tone is on, we still present food if the bird pecks a lighted key but when the tone is off, we reinforce pecks at a dark key. This is a classic conditional discrimination; food sometimes comes when the subject pecks

a lighted key and at other times when it pecks a dark key. Which of the two controlling relations prevails depends on the presence or absence of the tone.

The presence and absence of the tone may or may not be correlated with particular responses, but such responses do not enter into the specification of the reinforcement contingency. Nor would the identification of such responses help us define the function of the tone; the important relation here is not between tone and, for example, listening responses but between tone, a conditional stimulus, and key light, a discriminative stimulus. The light is sometimes positive and sometimes negative with respect to reinforcement, a relation that is determined by the presence or absence of the tone. Studies of listening and other kinds of observing responses have, of course, revealed important stimulus control variables (e.g., Dinsmoor, 1983; Wykoff, 1952), but in conditional discriminations, we are dealing not just with how stimuli are perceived or with relations between responses and controlling stimuli but with relations between controlling stimuli themselves.

Stimulus–stimulus relations, of which the conditional discrimination is an elementary example, take us one or more steps beyond the three-term contingency in the analysis of stimulus control. Understanding stimulus control is not just a matter of looking into how responses are hooked to discriminative stimuli but even more, requires the investigation of relations among stimuli. More about that later.

Originally, it was the inclusion of the environment in the definition of behavior that led behavior analysts to investigate stimulus control. Broadly defined, one may look at stimulus control as the development and maintenance of relations in which conduct and environment are tied together into analytical units (see,

e.g., Sidman, 1986). This view of the environment as a necessary element in the definition of behavioral units has stimulated behavior-analytic interest in many fields of research.

For example, psychophysics relates controlling properties of the environment to features of the sensory apparatus. Major behavior-analytic contributions to psychophysics were Blough's classic studies in which he taught pigeons to tell him when a spot of light became too dim for them to see it (Blough, 1956)—a remarkable achievement. Even now, I am not sure we do as well in sensory testing of humans with whom we cannot communicate verbally.

In pharmacology, chemical and other processes within the internal environment control the effects of drugs. Pioneer work by Dews (1955) demonstrated that reinforcement schedules can modulate those internal processes. This, of course, was a reversal of the standard assumption that behavior is simply a reflection of internal processes, and was a key event in the founding of a new science, behavioral pharmacology.

In neurology and neurochemistry, the central nervous system mediates relations between action and environment. Studies by my colleagues and me of stimulus control deficiencies in people who had suffered strokes would have been enough to convince me that behaviorists could gain much by working the central nervous system into their formulations instead of ignoring it. The tie-in between brain function and specific kinds of behavioral units was clear (e.g., Sidman, Stoddard, Mohr, & Leicester, 1971). These findings, although nearly untouched by subsequent investigators, remain as fertile ground for the derivation of methods to alleviate some of the devastating behavioral effects of brain damage. They also pose questions about how to account for certain features of stimulus control that behavior analysts have not yet attended to. How is

it, for example, that although words are made up of letters, some patients can match dictated words to printed words much better than they can match dictated letter names to printed letters (Sidman, 1971)? Also, how is it that in matching dictated to printed color names, some patients make the same errors with the names that they make in matching the actual colors (Leicester, Sidman, Stoddard, & Mohr, 1971); for example, in response to the dictated word "yellow," if they select the color orange, they also select the word "orange" instead of the word "yellow." Such problems require behavioral analysis, both experimental and applied.

And in cognition, some kinds of knowledge turn out to consist of behavioral units that are under the control of relations among environmental elements—four-, five-, and *n*-term contingencies (Sidman, 1986). In studying equivalence relations, behavior analysts may be showing cognitive psychologists the way. The power of reinforcement is not to be denied or minimized, but it is also clear that much stimulus control comes about without being directly generated by a reinforcement contingency and without any possibility of being a product of primary stimulus generalization. The concept of classes, and particularly of equivalence classes, provides theoretical and investigative tools for studying matters to which cognitive psychologists give names like *representation*, *reference*, *symbolism*, and *semantic meaning*. Perhaps behavior analysts can explain to cognitive psychologists why they and others say those words.

My own experimental interest in stimulus control started way back, long before the term *equivalence relation* came into my vocabulary. My master's thesis (Sidman, 1949) was concerned with some questions about Skinner's early definition of stimulus discrimination as an extinction process. After demonstrating that a reinforcement schedule (which
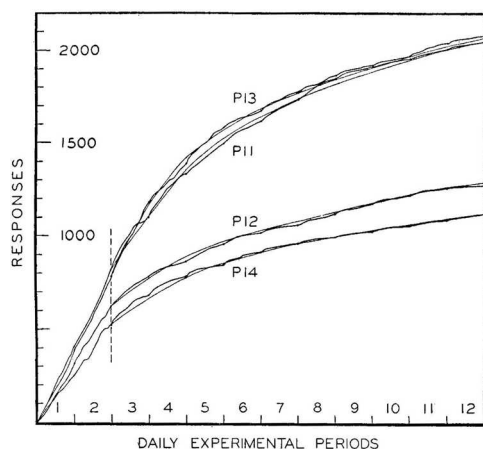
Figure 1. The development of a discrimination. Cumulative lever-pressing records (and smoothed theoretical curves) for 4 rats. At the vertical line, a discriminative stimulus was introduced whenever a reinforcement became available (every 5 min). (Skinner, 1938, p. 186)

at that time he called periodic reconditioning) yielded a stable response rate by individual animals, he performed a characteristically simple and ingenious experiment, as summarized in Figure 1, a set of daily cumulative response records for 4 animals. During the first 2 days, up to the vertical broken line, stable response rates were produced by a 5-min periodic reconditioning schedule. From the 3rd day on, whenever the schedule made a reinforcement available (every 5 min), a stimulus came on. As a result of this seemingly minor change, the subjects' rates of lever pressing in the absence of the stimulus (which he called S^Δ) gradually decreased during the next 10 sessions, but every time the stimulus came on (which he called S^D), the animals responded quickly. Eventually, they rarely pressed the lever except when S^D came on. Skinner remarked that this observation, along with a number of more quantitative considerations, "confirms our hypothesis that the process of discrimination is only a special case of extinction" (Skinner, 1933b, p. 338)

(note Skinner's use here of the term *hypothesis*).

One of the many questions Skinner subsequently asked was how to abolish a stimulus discrimination, how to eliminate the difference in behavior with respect to two stimuli, S^D and S^Δ. He demonstrated two methods (Skinner, 1933a). First, he showed that the response, already extinguished in S^Δ, can also be extinguished in S^D, thereby removing any evidence of discrimination. The second method was to recondition the response that had been extinguished in S^Δ, again removing any evidence of differential control by S^D and S^Δ but leaving the response intact in the presence of each stimulus. I did not believe, however, that the return of the response in S^Δ was in itself sufficient evidence that the discrimination had been abolished. After all, besides the weakening of the response in S^Δ, the formation of the discrimination had also involved the maintenance of the response in S^D. I felt that it was still necessary to investigate the possibility of control by S^D even after the reconditioning of the response in S^Δ.

Skinner reported some evidence on this point (1938, p. 195). After reconditioning the response in S^Δ, he demonstrated residual control by S^D after satiated animals had stopped responding in S^Δ. He dismissed this finding, however, with the relatively weak argument that induction from S^Δ would keep S^D a bit ahead in strength. My experiment showed no evidence of residual control by S^D after reconditioning in S^Δ, but methodological problems kept me from suggesting that Skinner was wrong. That was just as well, because it turned out that neither of us had been asking the right question. We were asking specifically whether a discrimination could be abolished by returning to the reinforcement conditions that existed before it had formed. We should have been asking more general questions: Should we be

distinguishing between the process of discrimination formation and the result of that process? Once a discrimination has formed, might we then have a unit of behavior that continues to exist independently of the method by which it was established? Was it possible that a mere return to the prediscrimination reinforcement conditions might not suffice to wipe out the discriminative function of S^D? Was it possible that even extinction of the response in S^D did not suffice?

Well, science sometimes progresses slowly. Those who are looking for a place to jump in might do well to read some old papers by productive researchers rather than just looking at currently popular research areas. Skinner's 1933 publications on the formation and abolishment of discriminations were not followed up until my 1949 thesis—which itself remained unknown, but deservedly so. The next directly relevant publication was by Barbara Ray in 1969, and she framed her questions in an original way that did not even recognize Skinner's earlier and more limited question, although they were definitely related to his later and more general formulations.

Without going into Ray's (1969) creative experimental techniques and her solid data, her rationale and conclusions carry us a significant step beyond Skinner's original questions about the abolishment of a discrimination. Ray quite cleanly demonstrated a distinction between the existence of a stimulus discrimination and the frequency with which that discrimination occurs. Noting that operant responses may change in their frequency of occurrence without changing their topography, she proposed that controlling stimulus–response relations, too, are topographical units that change their frequency of occurrence in response to the prevailing reinforcement schedule. Her concept of topographical units of stimulus control makes a clear

distinction between the formation of a stimulus discrimination as an extinction process, as Skinner noted, and the result of that process. When an organism learns to respond to a new stimulus, a new stimulus–response relation, a new stimulus control topography is created. This answers the question of where stimulus discriminations go when they are no longer reinforced or when some alternative topography is reinforced. They do not go anywhere; they simply occur less often.

This conclusion should have brought behavior analysts into an area of stimulus control where few of them have ventured, an area to which most psychologists, along with the person in the street, apply the structural concept of *memory*, but which I prefer to characterize behaviorally as *remembering*. The question to be asking is not where do memories go? but rather what were the original conditions under which the behavior took place? What stimulus control topographies were established? Then, to the extent that those conditions, those topographies, can be reinstated, people will remember things—the behavior will come back. The experimental consequences of this application of stimulus control topography seem obvious; what has behavior analysis been waiting for?

The most recent elaboration of the concept of stimulus control topography has been its application to a fundamental problem in stimulus control research, the identification of the controlling stimulus. Prokasy and Hall (1963) first stated this problem as follows:

What represents an important dimension of the physical event for the experimenter may not even exist as part of the effective stimulus for the subject. Similarly, the subject may perceive aspects of an experimenter event which have been ignored by, or are unknown to, the experimenter. (p. 312)

More recently, McIlvane and Dube (2003), in their analysis of what they

call *stimulus control topography co-herence*, have given a compelling account of the history of this problem, its current status, and its implications for future research and theory. In their words,

Stimulus control topography coherence refers to the degree of concordance between the stimulus properties specified as relevant by the individual arranging a reinforcement contingency … and the stimulus properties that come to control the behavior of the organism that experiences those contingencies. (p. 195)

We too often assume that what we as experimenters, clinicians, or just as observers of the everyday world see as the controlling stimuli actually do control the acts of any subject or person whose behavior we are trying to understand. The existence of stimulus–stimulus relations makes identification of the controlling stimuli especially complex. Higher order conditional relations provide contexts that determine when lower order relations are active and when they are dormant; stimulus control is not an always-or-never proposition. Contextual control makes the analysis difficult.

Sometimes a lack of coherence between the conceived and the actual stimulus control topography causes only minor problems. How many times have a baby's smiles delighted me as I marveled at its astuteness in recognizing an obvious grandfather type, and then disillusionment when the baby reached up and grabbed my eyeglasses? Sometimes, however, we can make serious mistakes in our science or in our personal lives by making unverified assumptions about controlling stimuli.

A scientific case in point is the type of conditional discrimination called identity matching. Carter and Eckerman (1975) suggested that *identity matching* can be a misnomer; for example, a sample and comparison that *we* specify as red may, for the subject, be as different from each other as, say, a red sample and a vertical
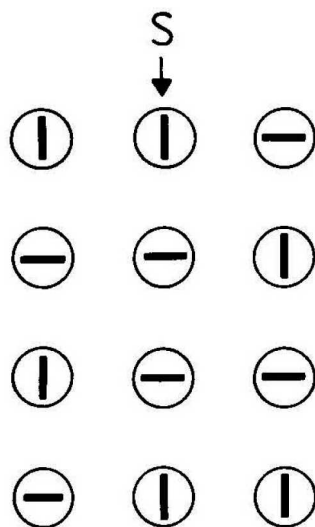

Figure 2. The four three-key stimulus configurations in horizontal–vertical tilt matching. The sample (S) was always on the center key.

comparison. A study by Iversen, Sidman, and Carrigan (1986) showed clearly that what the experimenters called identity matching was not identity matching for the subjects. The experiment used a standard three-key conditional discrimination procedure to teach monkeys tilted-line matching. Figure 2 shows the four three-key stimulus configurations faced by the monkeys on different trials. The sample, indicated by S, was always located in the center, and the comparisons were on the two outer keys. An animal produced reinforcers by touching the side key that matched the center key.

When the animals learned to perform this task almost errorlessly, the procedure was changed. From trial to trial, the sample might now appear on any of the three keys, and the correct comparison appears on either of the remaining keys. The left section of Figure 3 shows the original trial types, with the sample always in the center; the middle section shows the stimulus displays when the sample appeared on the left key, and the right section shows the sample on the
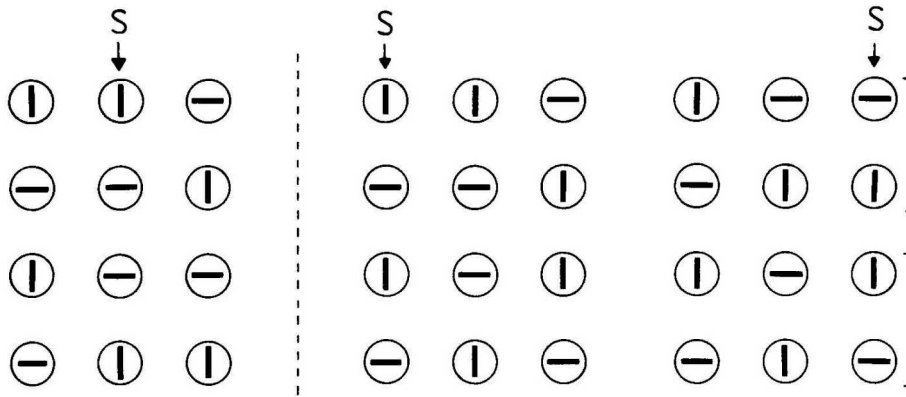
Figure 3. All of the three-key stimulus configurations when the sample (S) could appear on any key (Iversen, Sidman, & Carrigan, 1986).

right key. When sample and comparison lines appeared in novel positions, the conditional discrimination was completely disrupted. The controlling stimuli for the line discrimination were not just horizontal and vertical lines but horizontal and vertical lines on particular keys. An accurate specification of the controlling stimuli would have included these locations. Clearly, there had been a lack of coherence in stimulus control topography between subject and experimenters. The experimenters had been matching identical lines; the subjects had not.

Another finding in this experiment was that when the animals had learned to match identical colors rather than lines, changing the sample and comparison locations did not disrupt the conditional discriminations. What made the line orientations, but not the hues, susceptible to joint control by sample and comparison locations was undetermined. There are no principles in the experimental analysis of behavior that account for observations that different classes of stimuli exert differential control over behavior. Might there be analogues in human behavior, perhaps leading to differential sensitivities to certain stimuli by males and females, or babies and adults, or so-called right-brained and left-brained people?

All stimuli are, of course, inherently complex. If our subject responds to a key that has a line on it, we cannot tell on any particular occasion whether the response is controlled by the line's tilt, height, width, illuminance, distance from the edge of the key, or any of many other possibilities. Any single instance of stimulus control is, therefore, always an inference; the identification of a stimulus control topography requires many observations and many variations of possibly relevant stimulus features. We have to beware, especially, of attributing our own stimulus control topographies to our subjects and students. This point was brought home to me vividly when we taught monkeys and baboons to match vertical-line samples to green comparisons and horizontal-line samples to red comparisons (Sidman et al., 1982). In Figure 4, above the dashed line, we see the four possible stimulus displays during the teaching phase. In each display, we see the two color comparisons (red and green) above the line sample (vertical or horizontal). If the sample was vertical, the correct comparison was green; with a horizontal sample, the correct comparison was red. We then tested these conditional discriminations for symmetry, as shown in the four stimulus displays below the dashed line. That is to say, having learned to match the
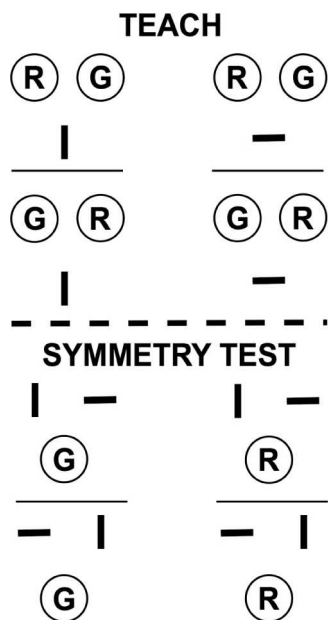
Figure 4. Above the dashed line: four stimulus displays presented during teaching to match tilted line samples to green (G) and red (R) comparisons. Below the dashed line: four stimulus displays during symmetry tests of the learned matching-to-sample performances.

lines as samples to the hues as comparisons, would the animals then also match the hues as samples to the lines as comparisons—green to vertical and red to horizontal?

Why not? You and I would certainly do that. Skinner seemed to have thought pigeons would, too. After having done an experiment (Epstein, Lanza, & Skinner, 1980) in which some pigeons learned to match color samples to symbols and others learned to match the symbols as samples to the colors, the authors stated that they believed the birds would be able to reverse their roles. Our monkeys and baboons, however, did not back up this prediction; they proved to be unable to perform conditional discriminations that were symmetrical to the ones they had been taught. Other reports in the literature indicated that pigeons would not do so, either. It was highly likely that we had not specified the controlling stimuli correctly, that the

stimuli the animals were matching were not the ones we had specified.

Again, sample and comparison locations were good possibilities as unintended controlling stimuli. As Figure 4 shows, in the symmetry tests all the stimuli were located on keys on which they had never appeared before. Here, though, another stimulus control complication was introduced. During teaching trials, the conditional discrimination required a simultaneous discrimination between the two comparison hues and a successive discrimination between the two lines; vertical and horizontal lines were never present at the same time. Then, during symmetry test trials, the colors had to be discriminated from each other even though they were no longer present for comparison at the same time. The lines, on the other hand, were now present together and for the first time had to be discriminated simultaneously. The differential stimulus control topographies generated by simultaneous and successive discrimination procedures will repay more study than they have received.

Finally, let us return to Skinner's original notion that stimulus discrimination is an extinction process. Skinner (1938) did go on to present data and concluded that "if the procedure of discrimination is introduced before either member [S^D or S^A] has been conditioned, the discrimination may be regarded as essentially complete at the beginning [that is to say, without any extinction being required]" (p. 206). This early conclusion was followed up by Terrace's brilliant demonstrations of errorless discrimination learning (Terrace, 1963a, 1963b). Terrace's groundbreaking studies led, in turn, to the development of programmed instruction (Skinner, 1968). The reality of errorless learning, brought about by the carefully planned teaching of all prerequisites before introducing something new to be learned, is perhaps one of the most important

contributions of basic behavioral science to the world outside the laboratory. Unfortunately, we have so far failed to crack open the doors of the educational establishment. I have often said that every school of education should have in front of it statues of Terrace's pigeons. It seems that we have yet to learn how to match the stimulus control topographies of educators to the topographies our science has taught us.

Well, there is so much more to be said. I have just picked out a few themes that have greatly influenced some of my own behavior, and I must apologize to the many whom I have not mentioned but who have nevertheless made—and continue to make—important contributions in the area of stimulus control. I look forward to hearing more in future state of the science presentations.

REFERENCES

Blough, D. S. (1956). Dark adaptation in the pigeon. *Journal of Comparative and Physiological Psychology*, *49*, 425–430.

Carter, D. E., & Eckerman, D. A. (1975). Symbolic matching by pigeons: Rate of learning complex discriminations predicted from simple discriminations. *Science*, *187*, 662–664.

Dews, P. B. (1955). Studies on behavior: I. Differential sensitivity to pentobarbital of pecking performance in pigeons depending on the schedule of reward. *Journal of Pharmacology and Experimental Therapeutics*, *113*, 393–401.

Dinsmoor, J. A. (1983). Observing and conditioned reinforcement. *The Behavioral and Brain Sciences*, *6*, 693–728.

Epstein, R., Lanza, R. P., & Skinner, B. F. (1980). Symbolic communication between two pigeons (*Columba livia domestica*). *Science*, *207*, 543–545.

Iversen, I. H., Sidman, M., & Carrigan, P. (1986). Stimulus definition in conditional discriminations. *Journal of the Experimental Analysis of Behavior*, *45*, 297–304.

Leicester, J., Sidman, M., Stoddard, L. T., & Mohr, J. P. (1971). The nature of aphasic responses. *Neuropsychologia*, *9*, 141–155.

McIlvane, W. J., & Dube, W. V. (2003). Stimulus control topography coherence theory: Foundations and extensions. *The Behavior Analyst*, *26*, 195–213.

Prokasy, W. F., & Hall, J. F. (1963). Primary stimulus generalization. *Psychological Review*, *70*, 310–322.

Ray, B. A. (1969). Selective attention: The effects of combining stimuli which control incompatible behavior. *Journal of the Experimental Analysis of Behavior*, *12*, 539–550.

Sidman, M. (1949). *The effects on a discrimination of a return to periodic reconditioning.* Unpublished masters thesis, Faculty of Pure Science, Columbia University.

Sidman, M. (1971). The behavioral analysis of aphasia. *Journal of Psychiatric Research*, *8*, 413–422.

Sidman, M. (1986). Functional analysis of emergent verbal classes. In T. Thompson & M. D. Zeiler (Eds.), *Analysis and integration of behavioral units* (pp. 213–245). Hillsdale, NJ: Erlbaum.

Sidman, M., Rauzin, R., Lazar, R., Cunningham, S., Tailby, W., & Carrigan, P. (1982). A search for symmetry in the conditional discriminations of rhesus monkeys, baboons, and children. *Journal of the Experimental Analysis of Behavior*, *37*, 23–44.

Sidman, M., Stoddard, L. T., Mohr, J. P., & Leicester, J. (1971). Behavioral studies of aphasia: Methods of investigation and analysis. *Neuropsychologia*, *9*, 119–140.

Skinner, B. F. (1933a). The abolishment of a discrimination. *Proceedings of the National Academy of Sciences*, *19*, 825–828.

Skinner, B. F. (1933b). The rate of establishment of a discrimination. *Journal of General Psychology*, *9*, 302–350.

Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1968). *The technology of teaching.* New York: Appleton-Century-Crofts.

Terrace, H. S. (1963a). Discrimination learning with and without "errors." *Journal of the Experimental Analysis of Behavior*, *6*, 1–27.

Terrace, H. S. (1963b). Errorless transfer of a discrimination across two continuua. *Journal of the Experimental Analysis of Behavior*, *6*, 223–232.

Wyckoff, L. B., Jr. (1952). The role of observing responses in discrimination learning. *Psychological Review*, *59*, 431–442.